follow that condition A, being now satisfied, will continue to be satisfied for all time?

If the answer be Yes, then of course $\frac{dH}{dt}$ will continue to be negative, until at length H reaches its minimum, and the system attains to perfection in the form of the Maxwell-Boltzmann law. If that is necessarily the future of our system, then, as Dr. Watson says, the Maxwell-Boltzmann law is not only a sufficient, but a necessary condition for permanence.

I am not aware, however, that this doctrine of (so to speak) final perseverance has ever been proved to be true. I do not think it can be regarded as axiomatic.

It seems to me that if we are to make our finite system reach perfection with any certainty, we must resort to the principle to which I appealed in my first letter on this subject—that every material system is constantly receiving disturbances from without, the effect of which is to keep condition A in working order, and so to make $\frac{dH}{dt}$ generally negative. Otherwise we must regard our system as only part of an infinitely extended system, the parts of which, when not too distant, mutually influence each other.

S. H. Burbury.

Research in Education.

It is quite unnecessary for Mr. D. S. T. Grant to suffer "dialectic annihilation" (see p. 5) in order to discover Prof. Armstrong's definite scheme of scientific education, inasmuch as in 1889-90 such a scheme was published by a Committee of the British Association, of which Dr. Armstrong was an active member.

As I believe many schools are still waiting for evidence as to the practicability of the scheme before adopting it, I venture to quote my own experience. I have been engaged for some time in practically applying this method to the teaching of girls of various ages, and am in a position to state that the scheme is perfectly workable.

It is not, of course, suggested that students should find out every known fact in chemistry or physics by a process of personal research—life is not long enough; but, if their early training be on these lines, they are in a much better position later on to accept, or if necessary reject, the work of others. A scientific method of thinking is of far more value than an accumulation of facts, and so it is extremely important that children should begin this kind of work before their logical perceptions have become obliterated by a continued application to irregular verbs. The problems set to young children are naturally of a very simple nature, and I do not leave the girls to themselves to "struggle to the truth by a process of trial and error." I state the problem to the class, and I usually find the girls have plenty of suggestions to offer as to its solution; these suggestions I criticise, and as soon as a practicable method has been found, the girls work it out for themselves. The early problems involve measurements of length, area, volume, and weight, and naturally the use of each new instrument is explained and illustrated. Simple physical problems follow these, such as experiments on relative density, and thus children are led to realise and appreciate the common properties of matter. After this training they are much more ready to solve elementary chemical problems. Certainly they could never work long enough to discover Dalton's laws for themselves, but they can quite appreciate classical experiments, and see how theory supplies an explanation of the facts. I am quite aware that if children are to work in this way they cannot be expected to sit still in their places with the look of passive receptivity on their faces, which is conventionally regarded as the proper appearance of well-disciplined scholars. They must move about, and should be encouraged to talk to each other about their work. I am convinced that a class of about eighteen is quite large enough if sound work is to be done; and if at any time their excitement becomes noisy, I find that a threat of numerical problems is quite sufficient to make them continue their practical work more peacefully.

It seems to me that physiology and hygiene, as usually taught in girls' schools, are absolutely pernicious and unscientific. Girls learn a list of the circulatory organs as they do the kings of England, and with less advantage. It would be considered criminal in them to doubt any of the facts in their books, although many are wrong, and yet, I take it, scientific training misses a great point if it does not engender a wholesome spirit of doubt. But the worst feature of all is the way in which girls are taught certain things in theory of the meaning of which they have not the faintest notion. They can tell one that water is H_2O, but the real significance of the symbol is perfectly unknown to them, and of course they are not able to understand it without some chemical training, in spite of the fact that some schools consider themselves very advanced and practical if the lessons are emphasised by the burning of hydrogen and the manufacture of oxygen. Numberless examples of similarly useless facts could be quoted, which are learnt under the name of hygiene—teachers, parents, and girls vainly believing that this is science. But all these facts are forgotten as soon as some examination is passed, and nothing is left behind; whereas a logical system of scientific training produces an effect on the mind which it is impossible to overrate. Surely the aim of education should be to produce not people who are full of facts, but those who can make the best use of the brains they possess, who are clear-headed, and able both to perceive and take advantage of opportunities that may be afforded them.

Central Foundation School for
Girls, London.
L. Edna Walter.

The Bibliography of Spectroscopy.

It will be within the recollection of many of your readers that, in the year 1879, a committee was appointed by the British Association to report on the state of our knowledge of spectrum analysis, and I was asked to undertake the preparation of a bibliography of spectroscopy from the year 1870. It was not thought necessary to begin at an earlier date, for a bibliography of the subject is to be found in Roscoe's "Spectrum Analysis." With the help of several members of the committee, lists of spectroscopic papers were prepared, and appeared in the British Association Reports for 1881, 1884, and 1889. In that year Mr. H. J. Madan kindly consented to join the committee, and as he was then resident in Oxford he was able to afford valuable assistance in checking the references, and the section of the list that was published last year is almost entirely his work, as I had found it impossible to spare the time to go to London to look up the references in the libraries. Mr. Madan is now living in Gloucester, and therefore out of reach of scientific libraries; he has, notwithstanding, shown his interest in the subject by making frequent visits to Oxford and London to continue the work. He finds, however, that the work is hardly practicable for one so far removed from the great centres; and my object in writing this letter is to ask if any one will volunteer to relieve him from this duty—that is, on the supposition that the list is of real use to workers on spectroscopic subjects. Many of the readers of Nature will be able to give valuable opinions on this matter, and probably to suggest improvements in the manner in which the list is drawn up.

It has been suggested that the four sections of the list should be rearranged and published as one continuous catalogue. The advantages of this for the purpose of reference are obvious; but from an estimate obtained last year, the cost of printing would not be less than £100. Dr. Tuckermann also very kindly proposed that the "Bibliography of Spectroscopy" drawn up by him and published by the Smithsonian Institution in 1888, should be incorporated with the British Association lists; this would very materially increase the expenditure.

Mr. Madan is quite willing to undertake gratuitously the literary work involved in the collection and rearrangement of the various sections. But the expense of publication is so great that the British Association can hardly be expected to bear the whole of it, although it is quite likely that a liberal grant might be made. Probably also grants might be obtained from other societies interested in the work, if it appears that the catalogue would be of special utility to those engaged in research. The balance might be met by a moderate charge for each copy sold.

Cooper's Hill, May 15.
Herbert McLeod.

An Aquatic Hymenopterous Insect.

No doubt many of your readers are aware that, in 1863, Sir John Lubbock gave an account of an extraordinary hymenopterous insect which he had observed swimming in a basin of water taken from a pond at Chislehurst. Another observer (Mr. Duchess, of Stepney) had also found a single specimen about the same time; then, in 1881, Mr. Bostock found one in some pond water at Stone, Staffordshire, since which time it does not appear to have been recorded by any one. I have searched many ponds for it year after year, but without success.

On Saturday, May 4, the Quekett Microscopical Club held one

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of its excursions in search of pond life, the neighbourhood visited being Totteridge and Mill Hill. Mr. W. Burton obtained two small phials of the water for examination, and the first pipette of water turned out into the trough contained a minute fly, which Mr. Burton kindly brought to me, when I immediately identified it as the *Polynema natans* (Lubbock, *Trans. Linn. Soc.*, vol. xxiv. 1864, p. 135, plate 23).

As this capture was, for the fourth time, the result of chance, Mr. Burton and I set out (May 6) to search for more specimens. After dipping our nets in and carefully examining the contents for over two hours, my patience was at last rewarded by seeing a beautiful female, struggling to free its wings from the mass of minute vegetation gathered in the dipping net. After a few hours more search, I found four males, which, together with the female, I transferred to an observation tank, where all soon disported themselves in the liveliest manner, swimming, or rather flying, under water for over four days, during which period they did not, to my knowledge, once leave the water. I have since obtained others, which are under close observation, and in course of time I hope to trace out their life-history.

Perhaps, owing to the microscopic dimensions of many of the *Mymaridæ* (Haliday), very few entomologists have paid any attention to this most interesting and fascinating family of beautiful "Fairy Flies," to whose industry we are no doubt largely indebted for our freedom from "blights" of many kinds. They are, indeed, mere specks, scarcely visible to the eyes of ordinary folk, and yet they have their place in nature.

I am inclined to think that when the type collection of the *Mymaridæ*, made by the late Mr. Haliday, has been thoroughly examined, this name *Polynema natans* will have to give place, so far as the genus is concerned. I hope that before very long we shall have figures of all the genera in this most interesting group.

FRED. ENOCK.

Halley's Chart.

I HAVE been much interested with the letter of Dr. L. A. Bauer in your last number, as I happen to possess a map, or chart, bound up with a number of Dutch, German, and French maps of the end of the seventeenth and the first years of the eighteenth centuries. The latest map with a date is 1704. This English map is evidently the same as 974 (4) mentioned by Dr. Bauer. It is entitled "A new and correct chart showing the Variations of the Compass in the Western and Southern Oceans, as observed in y^{e} year 1700, by his Ma^{ties} command by Edm. Halley." The dedication reads as follows, in Latin: "Majestati semper Augustæ Gulielmi III. D.G. Magnæ Britanniæ Fra. & Hib. Regis Invictissimi. Tabula hæc Hydrographica Variationum Magneticarum Index. Devotissime Consecratur a Subdito Humillimo Edm. Halley." At one side of the map is the following: "The curve lines which are drawn over the seas in this chart do show at one view all the places where the variation of the compass is the same: The numbers to them show how many degrees the needle declines either Eastwards or Westwards from the true North: and the double line passing near Bermudas and the Cape de Virde isles, is that where the needle stands true without variation."

The chart is in excellent condition, but has no name or printer on it. The only indication is "J. Harris, Sculp." The course of a vessel going from and returning to England is clearly marked.

THOS. WARD.

Northwich, May 27.

ON THE LINE SPECTRA OF THE ELEMENTS.

I THINK Lecoq de Boisbaudran was the first who called attention to the fact that the line spectra of the elements are by no means so irregular as they seem to be at first sight. He discovered the similarity in the spectra of the alkalies and alkaline earths, and pointed out how the lines in the spectra of these two families seem to be shifted towards the less refrangible side with increasing atomic weight. Mascart, in 1869, found two strong triplets of lines in the ultra-violet spectrum of magnesium, similar to the strong green triplet so prominent in the solar spectrum. He says: "Il semble difficile que la reproduction d'un pareil phénomène soit un effet du hasard; n'est-il pas plus naturel d'admettre que ces groupes des raies semblables sont des harmoniques qui tiennent à la constitution moléculaire du gaz lumineux? Il faudra sans doute un grand nombre d'observations analogues pour découvrir la loi qui régit ces harmoniques." But the wave-lengths corresponding to these rays were then not accurately known, and so the most interesting feature concerning the oscillation frequencies, or the number of waves which pass any fixed point in unit of time, remained unnoticed. It was later on shown by Hartley, that the differences between the wave-numbers of the three lines seem to be the same for all the triplets. This constant difference of wave-numbers repeated in a number of doublets, of triplets, and of more complicated groups of lines, has now been observed in the spectra of many elements. There are repetitions of doublets in the spectra of sodium, potassium, rubidium, cæsium, copper, silver, aluminium, iridium, thallium; of triplets in the spectra of magnesium, calcium, strontium, zinc, cadmium, mercury, manganese, and of more complicated groups of lines in the spectra of tin, lead, arsenic, antimony, bismuth. In all these cases the differences seem to be absolutely constant. For, notwithstanding the great accuracy with which Rowland has taught us to determine the wave-lengths, the law holds good. As an example, I give the list of doublets in the spectrum of thallium, according to Prof. Kayser's and my determinations. The number of waves passing a fixed point in unit of time, is equal to the distance the light travels in unit of time divided by the wave-length. If we measure the wave-lengths in vacuo, the distance the light travels is the same for all rays. We may then choose as unit of time, the time that light requires to travel one centimetre, so that the wave-number is simply equal to $1/\lambda$, λ being the wave-length in vacuo, measured in centimetres. In this manner, we get rid of the necessity of settling the velocity of light, which as yet has not been measured with anything like the accuracy with which the wave-lengths are known.

$1/\lambda$	Difference.	Estimated limit of error.
18684·2 26476·6	7792·4	0·32
28324·1 36117·1	7793·0	0·63
30952·1 38744·8	7792·7	0·74
33569·4 41365·1	7795·7	4·90
34217·7 42010·2	7792·5	0·90
34526·2 42321·4	7795·2	4·50
35372·1 43164·7	7792·6	1·20
36879·2 44671·0	7791·8	2·40
37503·0 45293·8	7790·8	2·70
38305·0 46096·8	7791·8	6·80
38663·3 46452·4	7789·1	7·30
39157·0 46947·3	7790·3	8·20

The mean of the twelve differences, assuming their weights to be inversely proportional to the square of the estimated limit of error, is 7792·5. When the wave-lengths are not reduced to vacuo, the differences are also very nearly constant, because the reduction alters them all nearly by the same amount. But it was a source of satisfaction to me, that the reduction brought all the deviations from the mean value well within the limits of error, whereas without the reduction the second difference had been just beyond the limit. These twelve doublets do not comprise half the number of wave-lengths that have been

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